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Umbellularia Californica, Nutt. Sylv. i. 87.
South Peak.
Salix lasiolepis, Benth. Pl. Hartw. 335.
Butte Pass.
Populus Fremontii, Watson, Proc. Am. Acad. x. 350.
Mahon's Landing, Feather River.
Quercus Wislizenii, ADC. Prodr. xvi². 67.
Lower slopes of South Peak.
Quercus dumosa, Nutt. Sylva. i. 7

At about 1000 ft. This is an oak of southern rather than northern California. It has been found as far north in the Coast Range as the vicinity of Clear Lake.

Aristolochia Californica, Torr. Pac. R. Rep. iv. 128.
Summit of South Peak.
Tritelia ixioides (Ait. f.), Greene, Bull. Cal. Acad. ii. 42.
On the slopes of South Peak.
Tritelia laxa, Benth. Trans. Hort. Soc. n. ser. i. 423. t. 15.
Sutter City and the Buttes.
Brodiæa capitata, Benth. Pl. Hartw. 339.
Butte Pass.

On the Citing of Ancient Botanical Authors.

By N. L. Britton.

My review of Professor Greene's "Flora Franciscana" (BULLETIN, this volume, pp. 158-160), has been made by him the text of a most scholarly paper in the recent pages of his "Pittonia." His explanations, arguments and criticisms of my remarks are most skillfully drawn, and it is with a feeling that I have but little chance to win in the friendly dispute that I again essay to break a lance with him. However, it is perhaps desirable that I should have another word before my position shall have been rendered entirely untenable.

I have not accused my friend of originality in attributing generic names to the ancient Greeks and Romans. I am well aware of the truth of his statement, that he has not "entered upon course of procedure which has not been taken by learned and famous botanists at one time and another within the last century," even so lately as Baron F. von Mueller's "Second Systematic

Census of Australian Plants," published in the year of grace 1890; and it has been my fortune to meet during the past summer a scholar who must be freely accorded the distinction of being quite as shrewd in perception and as accurate in judgment as any who have preceded him, who holds to the same opinions.

I am led to write this, because Professor Greene has thought it desirable to disclaim the origination of the views he advocates and to refer to what we have had so recently and satisfactorily defined as "authority."

I will first consider his criticisms on my remarks. He takes exception to my statement that "Generic names used by pre-Linnæan authors and adopted by Linnæus are credited to the old writers even as far back as Dioscorides and Pliny," saying that it is true only under important limitations, and showing that he has adopted only those which have been satisfactorily identified. By this I understand him to imply those which he considers to have been satisfactorily identified. Thus he attributes *Cercis* to Linnæus because "the most eminent critics of ancient botany failed to identify *Kerkis*;" and again, "*Lotus* is credited to Tournefort because the ancients had several Loti." And yet my sentence is I think actually true, for I did not say, nor intend to say, that "All generic names used by pre-Linnæan authors and adopted;" etc. But we will not quibble over a word. I might better have said, "many generic names, etc.," and will credit him with a point of vantage.

But I think that he has here touched on a very weak place in his armament, and one that I did not allude to in my review because I realized that I was approaching dangerous ground, and recognized the necessity of keeping some things in reserve. The point that I would make is the undisputed uncertainty of identification of a very large number of ancient names. Not all ancient names, but a great many. There is no preservation of the plants of Dioscorides, Theophrastus or Pliny. Some of them can be known from description, some from ancient engravings, some of them can not be known at all, and between the knowable and the unknowable there is a hazy region which each critical student will interpret differently. Now how in the world are we to attain stability under the system advocated by Prof. Greene, while such conditions exist? *Per contra,* if we begin to reckon genera from some author, all, or very nearly all, of whose genera may be known by the preserved types, we at once dispel the preceding haze and remove at once a very grave difficulty.

My friend next considers my statement that "The ancients did not use the names in the generic sense of Tournefort, Linnæus and others of about their time, but, in most cases at least, as mere appellations for plants." Inasmuch as he admits "the impossibility that the ancients should have applied names in the generic sense of any generation of modern botanists," the only fact which I wished to emphasize by this sentence, it is hardly worth while to dwell on it longer. There is nothing in it to imply that they never applied names as generic. But his remarks open up the very broad field of research into the history of the evolution of Systematic Botany, which he is so admirably equipped to discuss, and about the details of which I am so ignorant. For, like everything else in the universe, the science has been produced by little and little. I suppose that primitive man, being brought as intimately as he was into contact with the vegetable kingdom, early came to know such plants as were useful to him for food and clothing, and must have had names for them which were transmitted—doubtless with many modifications—from generation to generation. As the faculties of perception became developed, it became evident that there were relationships among the plants, and by a closer and closer study of these relationships the knowledge of the subject has increased to the present. Theophrastus, Pliny and Dioscorides have, I know, been called the "Fathers of Botany," and in its broad sense the title is well given, but to my mind they are very far from being the Fathers of Systematic Botany. But if we consider other branches of human knowledge we find that it has not been found possible to push practical procedure therein back to anything like their beginnings. It has been found necessary to establish by common consent "statutes of limitation," beyond which inquiry and speculation very freely go, but beyond which practical knowledge cannot be reached, and I believe it necessary in our science to establish a statute of limitations at a practical, available point, and this point I believe to be where the writings of some author who proposed a complete system by means of which plants could be grouped, and whose conclusions may be verified and have been verified by examination of the materials on which his results were based. To the statements that " few or any of the names are likely to be older than the Latin or Greek languages in whose vocabularies they are found," I must therefore take decided exception.

A point of departure such as I have indicated, being then in my opinion a necessity of the case, it will be seen that if my views are to be maintained, this must be found at the writings of either Tournefort or Linnæus, the systems they established being complete in the light of the information they possessed, and their genera practically every one recognizable from their books or herbaria, and even should the latter be destroyed, the knowledge of their genera is so widely diffused that there would never be any uncertainty about them. Tournefort's system is a generation older than that of Linnæus, and if there were no other circumstances to be considered, would have overwhelming claim to the distinction. But there is a very grave difficulty in adopting it, and one which I believe will ever render it unavailable. It is this : It is of the very highest degree of importance that the two sciences of botany and zoology should go hand in hand, and the zoologists can never go back to Tournefort, because he established no animal genera. The versatile Linnæus, however, brought together all Natural History in his Systema Naturæ. He was familiar with plants, animals and minerals. I doubt if it can be satisfactorily maintained that he was more botanist than zoologist. He must have known the whole range of nature better than any other man of whom we have any record. The collections which he brought together were actually immense for his time, and his library was of the richest character. Besides all this, he certainly established the binomial system of nomenclature, and its general acceptance dates from his time, no matter what authors may have used it before. Tournefort did not use it at all, so we cannot go to him for species, another consideration which with me is a strong argument against going to him for genera. And so, as I said in my review of my friend's work, it seems better to me not to go back of Linnæus.